PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	The prevalence of type 2 diabetes mellitus in women of childbearing age in Africa during 2000-2016: A systematic review and meta-analysis
AUTHORS	Chivese, Tawanda; Werfalli, Mahmoud; Magodoro, Itai; Chinhoyi, Rekai; Kengne, AP; Norris, Shane; Levitt, Naomi

VERSION 1 - REVIEW

REVIEWER	Dr Salwa Zghebi University of Manchester, UK
REVIEW RETURNED	14-Aug-2018

GENERAL COMMENTS	Comments for the Authors The paper presents a comprehensive and well-designed systematic review and meta-analysis for estimating prevalence rates for type 2 diabetes in women of childbearing age in Africa between 2000 and 2016. Originality This is potentially an important study assessing the prevalence of type 2 diabetes in African women. The research question is well-defined and based on protocol published previously. Comments 1. Eligibility criteria: the authors need to justify the selection of this study period. Why only included studies from 2000 onwards? 2. In the data synthesis and analysis section, the authors list the age groups found in the included studies. These are results and would be better placed in the Results section. 3. It is not mentioned in the Methods on the plan to only include studies reporting age- and gender-specific rates in the meta-analysis. This is mentioned in the Search Results section, but need to be mentioned earlier for clarity. 4. Table 1 is mispecified on page 10, Line 8. i.e. it is not matchgin the Table 1 presented on pages 11, 12. 5. On page 11 line 4, the authors state "A total of 39 studies, from 27 countries, with 52 075 women of child bearing age….." but also state "A total of 81 studies from 39 countries were included, totaling 52 075 participants" in the Abstract. Please revise these inconsistent statements. 6. Please describe briefly of how the ROB score in Appendix 2 was computed. 7. In Discussion (Line 18-32) the authors elaborate on possible utility of FPG testing, but it would be useful to discuss the potential role of HbA1c testing.

	8. There are few typos e.g. "80 included studies..." Page 10, line 8; page 15 Line 23; page 19 Line13. Please revise. 9. The URL provided in reference #5 is outdated. Please update.

REVIEWER	Dr S Bellary Aston University,UK
REVIEW RETURNED	22-Oct-2018

GENERAL COMMENTS	Thank you for asking me to review this manuscript. Maternal diabetes has a great impact on maternal health as well as risk of diabetes in the offspring and therefore huge implications for population health. Given the projected increase in diabetes in the african continent this work is highly relevant especially considering the scarcity of data from this part of the world. The objectives are well defined and the outcomes are well stated. It would have been good if the GDM studies were included as well given that the T2DM,IFG and IGT may not fully capture the extent of risk of maternal diabetes. The methodology is sound and thorough. However, there are a couple of points. Firstly, the sample size of some studies (using a cut off of 100 subjects) is highly variable and i wonder if a higher cut off e.g. 1000 patients would have been more appropriate considering these are prevalence studies and a small sample size can have an overall effect especially when assessing pooled prevalence. Secondly, although the intention to include all african studies is understandable, there is a huge variation in the geographical prevalences e.g. very high in south africa. Clearly, prevalence varies with regards to economic status of these countries and again it is likely that the heterogeneity may be due to the huge economic variation between the countries. How representative would the results be under these circumstances . Could a analysis be undertaken based on the economic status of the different countries ? It is interesting that the prevalence of T2DM, IGT and IFG are almost similar. Ideally you would expect a higher prevalence of pre diabetes states . Are there any reasons for this deviation from an expected pattern? In general results are well presented and clear. The discussion is also well written but it would be good of some of the points raised above are adequately addressed.

REVIEWER	Alipasha Meysamie Tehran University of Medical Sciences IR Iran
REVIEW RETURNED	12-Nov-2018

GENERAL COMMENTS	The abstract is too concise need more explanation specifically in results and method parts. In the results the number of included studies mentioned 80, however in the abstract and method it is reported as 81.

	Precision of results are different in abstract and in results, it shall be the same with at least 0ne decimal reported in all parts. The prisma diagram background is dark and not so good for publication. The other sources in the prisma diagram shall be defined. In the prisma diagram only 47 of exclusion reasons explained, 48 named to be excluded and remained is 80 out of 129 articles. This part shall be revised and corrected. Inclusion of Hird 2015 and Erasmus 2014 studies in IGT analysis in age group of 25-34 is not statistically acceptable and this analysis shall be performed without these groups. The overall measure reported for IFG and IGT because of variation in age between studies is not acceptable. No logic for pooling of these variable data can be acceptable. Forest plots of T2DM according to the age groups and urban and rural reports shall be included. In Appendix 1 – List of included studies, some %s are missed, some data also missed. Needs completion.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1 1. Eligibility criteria: the authors need to justify the selection of this study period. Why only included studies from 2000 onwards?	We included population based cross-sectional studies, published since the year 2000 as older studies would not have use the WHO 1998 T2DM diagnosis guidelines	Page 6 manuscript
2. In the data synthesis and analysis section, the authors list the age groups found in the included studies. These are results and would be better placed in the Results section.	Done	Manuscript page 8 and 9
3. It is not mentioned in the Methods on the plan to only include studies reporting age- and gender-specific rates in the meta-analysis. This is mentioned in the Search Results section, but need to be mentioned earlier for clarity.	Done	Manuscript page 6
4. Table 1 is mispecified on page 10, Line 8. i.e. it is not matchgin the Table 1 presented on pages 11, 12.	Corrected to Appendix 1	Manuscript Page 9, line 8
5. On page 11 line 4, the authors state "A total of 39 studies, from 27 countries, with 52 075 women of child bearing age….." but also state "A total of 81 studies from 39	The incorrect statement in the Abstract has been corrected in	Manuscript page 2, line 23, and Scholar One, Abstract

countries were included, totaling 52 075 participants" in the Abstract. Please revise these inconsistent statements.	both the Manuscript and in Scholar One	
6. Please describe briefly of how the ROB score in Appendix 2 was computed	A legend has been added to the ROB table with a brief explanation and link to a table with more detail	Supplementary figures and tables, page 12
7. In Discussion (Line 18-32) the authors elaborate on possible utility of FPG testing, but it would be useful to discuss the potential role of HbA_{1c} testing.	Done	Manuscript page 16, lines 1-5
8. There are few typos e.g. "80 included studies..." Page 10, line 8; page 15 Line 23; page 19 Line13. Please revise.	Done	Revised
9. The URL provided in reference #5 is outdated. Please update.		
Reviewer 2 The objectives are well defined and the outcomes are well stated. It would have been good if the GDM studies were included as well given that the T2DM,IFG and IGT may not fully capture the extent of risk of maternal diabetes.	We agree with this, although we didn't investigate GDM prevalence as there are two recent excellent systematic reviews that have already done so. We commented on this on page 17, lines 9-16	Manuscript page 17, lines 9-16
The methodology is sound and thorough. However, there are a couple of points. Firstly, the sample size of some studies (using a cut off of 100 subjects) is highly variable and i wonder if a higher cut off e.g. 1000 patients would have been more appropriate considering these are prevalence studies and a small sample size can have an overall effect especially when assessing pooled prevalence.	We also agree with this comment. However, we included smaller studies due to the scarcity of data available. We have added a	Manuscript page 17, lines 22-23

	comment under limitations about this.	
Secondly, although the intention to include all african studies is understandable, there is a huge variation in the geographical prevalences e.g. very high in south africa. Clearly, prevalence varies with regards to economic status of these countries and again it is likely that the heterogeneity may be due to the huge economic variation between the countries. How representative would the results be under these circumstances . Could a analysis be undertaken based on the economic status of the different countries ?	We acknowledge that variations in economic development may explain some of the heterogeneity across the studies and that our estimate may not be representative of the prevalences across the African continent due to this. We have added this under Discussion – Limitations	Manuscript page 17, lines 26-29
It is interesting that the prevalence of T2DM, IGT and IFG are almost similar. Ideally you would expect a higher prevalence of pre diabetes states . Are there any reasons for this deviation from an expected pattern?	One possible reason could that most studies did not measure and report these impaired glucose states and therefore these estimates are far less precise than that of T2DM	
Reviewer 3 The abstract is too concise need more explanation specifically in results and method parts	Done	Manuscript page 2 and Scholar One – Abstract

In the results the number of included studies mentioned 80, however in the abstract and method it is reported as 81	Done	Manuscript page 2 and page 7, line 14, and Scholar One – Abstract
The prisma diagram background is dark and not so good for publication. The other sources in the prisma diagram shall be defined.	Corrected	Fig 1
In the prisma diagram only 47 of exclusion reasons explained, 48 named to be excluded and remained is 80 out of 129 articles. This part shall be revised and corrected.	Corrected	Fig 1
Inclusion of Hird 2015 and Erasmus 2014 studies in IGT analysis in age group of 25-34 is not statistically acceptable and this analysis shall be performed without these groups.	The meta-analysis for IGT has been removed	
The overall measure reported for IFG and IGT because of variation in age between studies is not acceptable. No logic for pooling of these variable data can be acceptable.	The meta-analysis has been removed and replaced with a qualitative description	Manuscript page 13, lines 9-18
Forest plots of T2DM according to the age groups and urban and rural reports shall be included.	The forest plots are included as supplementary files	Supplementary tables and figures
In Appendix 1 – List of included studies, some %s are missed, some data also missed. Needs completion.	The missing data were missing from the studies. A legend has been added to the Appendix to this effect	Supplementary tables and figures

VERSION 2 – REVIEW

REVIEWER	Dr Salwa Zghebi University of Manchester, UK
REVIEW RETURNED	23-Jan-2019

GENERAL COMMENTS	The authors have addressed all my comments except 2 minor outstanding comments: a typo on page 19 and adding a link to

	reference 5. I believe these minor edits will be dealt with during the production stage anyway, so I will not raise these points that not necessitate another submission. Kind regards.

REVIEWER	SRIKANTH BELLARY Aston University Birmingham, UK
REVIEW RETURNED	10-Feb-2019

GENERAL COMMENTS	Thank you for asking me to review this manuscript. The revised manuscript is a much improved version over the original submission. Although there are still some limitations, they have been acknowledged. Most comments from the reviewers appear to now have been addressed.